THE PUBLISH OR PERISH SYNDROME[t]

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Abstract

Certain effects of the pressure on scientists to publish research are discussed, and a suggestion is made for amending the mode of publication in a manner which might alleviate some of these effects.

1. Introduction

Experience during the last four years as an associate editor of a scientific journal has led me to ponder upon the whole complex of the publishing process insofar as research is concerned. Although derived from this experience, the opinions expressed in this paper are entirely my own and in no way represent any official opinion of the journal I have been associated with, its editorial board or the professional body that sponsors it. Furthermore, nothing is intended as criticism of that (or any other) journal, its authors or referees, nor of my colleague associate editors or my editor. In fact, this paper has been cleared with him to ensure that it is not transgressing any implied confidences arising from this editorial work. The impetus behind the paper is that certain unfortunate features of the publishing of research papers ought to be aired in public more often than they are, in the hope of one day alleviating them. At the end of this paper a tentative suggestion is made for one possible way of changing the present mode of publishing research results which might reduce these undesirable features. As a first proposal it can scarcely be

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[*] On leave from Cornell University, March-April, 1971.

[t] Based on a paper given to the Southeast Texas Chapter of the American Statistical Association, College Station, Texas, on April 2, 1971.

be acceptable, but if it sets others to thinking about the problems and to suggesting alternative proposals that ultimately do yield something acceptable, then it may have served a useful function.

2. Reasons for publishing

What is the purpose of publishing scientific papers? The dissemination of knowledge, in most cases, of newly-found knowledge. The original form of scientific journals as we know them to-day was personal correspondence (oft-times lengthy) between individual scientists (the letters between Pascal and Fermat on problems in probability are a good example). The purpose of this correspondence as is the purpose of to-day's journals, was the distribution of knowledge, to serve as an aid to the development of further knowledge, for to achieve this development, a research scientist wants to be aware of all presently available knowledge in his field so that he can make a step forward in expanding that knowledge. This is the historical, and surely still the most important, reason for the publishing of scientific articles - the rapid dissemination of new knowledge to fellow scientists.

Nowadays, however, there seems to be another reason for scientists wanting to publish: the acquisition of status, as a step for example, towards gaining salary raises and professional stature. This arises from many employers evaluating their scientists in large part by their published works. There is nothing new, of course, in judging a man in any walk of life by the quality of his ideas, especially his new ideas. However, doing this too much by means of his published papers is, I believe, a procedure that bears examination because it is leading to the publish or perish syndrome - which, whether we like it or not, is now part of the lives of many scientists, especially among the younger ones.

To some extent, evaluating a man's quality by his publications is not an unreasonable practice. After all, in a university that grants tenure an administrator needs to have some assessment of the likelihood with which to-day's 28-year-old assistant professor will, in 25 years time, still be as on top of his subject then as he is now; and be able to do as good a job

then as he does now of teaching the relevant and up-to-date parts of his subject. In other words, the institution very reasonably wants to assess a man's ability to keep abreast of his subject. For want of other procedures, looking at publications is one of the few concrete things that can be done. But, unfortunately, this look so often appears to be largely a matter of just counting. Yet this, too, is understandable, for what else can a biochemist turned vice-president for academic affairs do when asked to pass judgment on a linguist or an architect, say? After all, publication of papers is a hallmark of peer judgment, and presumably of some sort of quality, and therefore deserving of recognition.

So it seems that we must truthfully, though sadly, admit that publication of papers is striven for not only to disseminate knowledge but also to allay the publish or perish syndrome. However, in accepting that we cannot eliminate the opportunity for value judgments being made on the basis of publications, maybe we could change the present mode of publication so as to both alleviate some of its undesirable features and to dilute the syndrome which accompanies it.

The publish or perish syndrome affects people in various ways. One of the most evident consequences among many university people is the devotion to research, at the expense of teaching. This is something that undergraduates have complained about in many places for several years - for example, courses being taught by graduate students rather than a professor, who is busying himself on research. Not that graduate student teaching is necessarily poor, far from it and indeed the opposite is often true; in addition, teaching can also be a most valuable part of a graduate student's training. But undergraduates who attend a university expecting to attend courses by its noted professors are often disappointed should those courses be given by graduate students substituting for the professor, who is busy on research. The publish or perish syndrome may also be contributing in similar fashion to the slow growth of teaching techniques that involve the computer. Developing and implementing such techniques (interactive computing facilities, for example) demands much time and effort, which the untenured professor may not be willing to give, at the expense of time available for research and the preparation of

papers which he can publish and so not perish. Another consequence of this desire of a professor to always be doing "his own thing" may be curtailment of time spent in reading and in keeping up to date not only with his specific research interests but also with the broader topics of his courses - particularly of undergraduate and service courses. Teachers, we must remember, are surely supposed to read extensively, but how many of us regularly sit down to some steady reading each day?

3. Preparing papers for publication

One often hears that so and so "is working on a paper". Does this sometimes mean that the would-be author feels he must get a paper published and so he's "working on a paper"? For many young scientists I'm sure this is so, because the pressure to publish, as a means of professional improvement, is itself very real. Consequently many a young scientist may find himself working on a topic chosen not because he's deeply interested in it but because he might "get a paper out of it", to use another unfortunate but oft-heard phrase. Nevertheless, this may be necessary if he feels he should maintain a box score of, say, 2 papers a year in order to publish and not perish.

Numerous consequences may arise from this state of affairs. One is the planning of short projects that lead fairly promptly to something of publishable form. Another can be the piecemeal publication of results of a long project - a note here, a short paper there, an abstract somewhere else, all with occasional use of the phrase that this or that aspect of the subject "will be reported elsewhere" - or "subsequently". Aside altogether from quality, this atomizing of published research results is, at least partially, a direct consequence of the publish or perish syndrome - and a distasteful one at that. Evidence of its existence is all too plain. Some journals already have a clearly stated policy of publishing short (4-page) articles more promptly than longer ones; and examples of the same author having 2 articles on the same topic, back to back, in the same issue of the same journal are also well known.

Furthermore, an extensive piece of work that someone has devoted many years to is often no longer reported as such, authors sometimes being asked to cut a long paper into 2 separate papers (which may nevertheless appear in the same journal). Not only are research results atomized in this fashion, with consequent loss of opportunity for the author to tie all his results together, but the individual pieces may get published in a variety of journals - maybe all over the world. As a result, it soon becomes a hopeless task to keep up to date with one man's work, let alone with all the work being done in any particular topic. Of course spreading one's papers over a wide variety of journals certainly achieves widespread dissemination of knowledge; but, cynically, it is also good for the author's ego.

4. Referees are human

After being sent to a journal how does a paper get accepted for publication? The answer is well-known: by being refereed and gaining favourable reports from the referees. What a sweepstake this is! And it is so for the following reason: not because of personality conflicts between authors and referees, not at all, but because to-day's volume of literature is getting so large that few referees can so definitively know a topic and all of its literature that they can always give a 100% factually correct refereeing critique, especially with regard to whether or not the material has already been published elsewhere. A referee can only do his job to the best of his ability, and we can, I'm sure, rest assured that it will be done in this manner, and impartially so. But with the steady increase of journal sizes and the burgeoning of new journals few people, even in their own areas of specialization, can always guarantee that they are fully cognizant of everything that has been published in those areas and hence neither can they always guarantee that what they are refereeing may not have been already published elsewhere. Furthermore, those best suited for refereeing are often too busy to do it and decline, or do not do it promptly and, with papers of great complexity, are often quite unable to make time to check all the details.

There is little blame here upon the people concerned, for these are consequences of our mode of publication, the demands of the refereeing that it entails and of the great weight we put upon publication vis à vis the publish or perish syndrome. The fault is with the system and I believe it needs amending.

In addition to the ability of a referee to do his job there is also the problem of the ability of the man who selects referees. How well does anyone know his profession, its subject matter and its participants, to be put in the position of deciding who shall referee papers? Not that editors are biased in their choice of referees; apart from anything else their task is too onerous for that. But editors and their associates can only use as referees those people whose names they know as being interested in the topics dealt with in submitted papers. And no editor, even in a limited number of topics, can know of all the experts, especially among those who have recently finished their doctoral dissertations and should be well informed. For this reason, changes in editorial staff every few years is essential, for new editors bring with them changes in the sample of people called upon to act as referees.

So far as the referees' work is concerned, true it is that occasionally a referee may seem to an author to be unreasonable in his criticism of a paper. But, being human, no referee is infallible, and probably most examples of unreasonableness are much more evident to editors than authors. Examples are the 30-page paper returned with no comment at all other than a recommendation to publish (surely no paper of that length can be perfect!); or the paper which, after 6 months of waiting and several reminders to the referee, comes back from a foreign country by sea mail, taking an extra 3-5 weeks to arrive; or the paper that never comes back at all, not a word from the referee, no letters, no report, nothing. Fortunately these transgressions are relatively rare and editors mostly receive referees' reports to be forwarded to authors in the usual way.

Upon receiving his paper back from refereeing, an author, realizing that referees are human too, must be aware that he has every right, indeed a responsibility, a very serious responsibility, to reply to a referee's criticism. It is my belief that for each and every revision a referee suggests the author should either make the necessary changes or write a clearly reasoned account

of why he declines to do so. Only then does an editor have opportunity to judge fairly between author and referee - and only by doing this is an author putting himself in a fair position for earning acceptance of his paper. Proclaiming loudly that the referee is wrong, or ignoring most of his comments and hoping the editor will overlook them too - neither of these responses to the referee's criticism will persuade an editor of the referee's apparent ineptitude. And yet it is surprising how many authors behave this way - particularly in ignoring criticism and hoping an editor will also.

5. Authors are human, too

If authors sometimes feel that referees are unreasonable, then editors certainly find that some authors are also. Consider the paper that came for refereeing which, on its first page, had the super-heading "Draft hand-out for annual meetings"! It was just that, a draft, of disconnected phrases. It is hard to believe that an author could be so slipshod. Another author recently wrote two papers on the same topic, with different titles, and sent them to two journals. By chance, both journals picked the same man as referee - imagine his surprise, and the author's chagrin at being caught, red-handed. Then there is the author who took more than three years to revise his paper but on resubmitting it requested that it be given speeded up refereeing; and the author who, on submitting his paper, wrote that it was considered so important that he hoped it would receive preferential treatment. Finally, there was the author who was told to substantially reduce his paper including deletion of certain paragraphs. His revision appeared to do this but it did not bear close examination. The paper had been re-typed, with narrower margins and with many of the displayed formulae put into the text. And the paragraphs recommended for deletion were still there, but in a different sequence and with their first and last sentences altered, presumably as a guise to fool the referees and editors! It nearly did, too.

These, then, are some of the problems of referees and authors as editors see them. How about the journals themselves?

6. To-day's journals

Most journals to-day are receiving an ever-increasing number of submissions, and this is bound to continue as more and more people enter research, many of them trying hard to publish and not perish. As a result there is pressure on journals, I think, to increase in size, to change their emphasis and to expand their interests.

There is also another problem, to my mind, which stems from the manner of journal publication as we have it to-day. This is the problem of expense, of both time and money. The delay time between typed manuscript and published article and the dollar cost of achieving publication are both increasing. A delay of 12-18 months between typed manuscript and published article is nothing out of the ordinary. By then the paper has been refereed, revised, maybe refereed again, copy edited, type set, galley proofed, printed, bound and mailed to 2, 5, or 10 thousand subscribers, or however many there are. Two facets of cost arise here. The first is the time (and effort) involved in the refereeing and revising; and the second is in the printing and mailing of thousands of copies of journals of 200 pages or so, effectively small books. This process is clearly very expensive of time and money. A pertinent question, therefore, is "How many subscribers really thoroughly and meticulously read any particular paper?"

For any particular research paper, which by definition is usually on a highly specialized topic, the number of readers who give it a thorough going-over is probably very few. Maybe as few as twenty in some cases, perhaps fewer, and with many papers probably always fewer than 100. In any case, there most surely would be far fewer than is warranted by our present procedures for disseminating such knowledge. True, our refereeing procedure does provide, despite what has been said, some reasonably efficient selection process, efficient in terms of keeping out much of the rubbish (the draft hand-outs for meetings). But it is not efficient in terms of total cost in time and money to our various professions. Not only is the published form expensive to produce, but it is expensive for each one of us to store thick journals most of which we will never read in detail.

The present system also contains aspects that are of debatable value to the specialists who will read a particular article in detail. They will read it with a fine toothcomb and, despite the best refereeing in the world, will find the error or two that inevitably slips by. Not that this will worry them because, through being concerned about the topic, they will understand all the details and be interested in them and will happily pardon minor errors. So if to the specialists, the people with the greatest stake in a paper, if to them the paper had been good in the first place, it is likely to be not much better in its printed form - and its taken 18 months to get it that way. On the other hand, if the paper was poor to begin with and had been published that way with little refereeing, the specialists would soon see it for what it was worth and not worry about it. So why go to the trouble of refereeing in the manner we presently demand?

Now of course we cannot do away with refereeing altogether. Journals would soon double and treble in size and information pollution would be more rampant than it is now. However, without refereeing and its implied peer judgment by which administrators evaluate people, it could be that the initial flood of papers would pass and the publish or perish syndrome might subside. Scientists would then revert to publication only when they had something worth saying. Generally speaking, though, there is a reasonable fear that the literature would become more polluted than it presently is. One thought here, though, is that if all published articles had to be paid for, the present potential level of information pollution might be reduced - except for the obvious flaw in <u>this</u> idea that wealthy institutions might then dominate the literature. Furthermore, any complete abandonment of refereeing would be particularly dangerous for practitioners who just want to use published results, accepting them on faith because they are in print. And in this way refereeing does cut down on information pollution, but we must remember that it is far from foolproof and is costly of time and money. However, perhaps amending its present form along with amending the mode of publication could improve the dissemination of knowledge, with less cost in time and money and with a diminution of the publish or perish syndrome.

7. A suggestion

No first suggestion for changing refereeing and publication procedures can hope to be acceptable as it stands, but maybe it can serve as catalyst for designing some long-term improvements that will be acceptable. In this context, therefore, the following suggestion is made. Only two kinds of papers would be published: extended abstracts of research papers, and full length review-style papers.

Instead of publishing full-length research papers that have, hopefully, been refereed in all their grisly detail, only 2-3 or 4-page abstracts would be published. Full-length papers, corresponding to each abstract, would be available on request for a small fee, and would be kept available for a long period of time. These full-length papers would not only be the same nature as, but could also contain more detail than, to-day's published papers. By containing more detail (where appropriate) they could, to many readers, be more useful and they could also make a referee's task more straightforward. In the refereeing process, a referee would see both the extended abstract and the full-length paper and would be entitled to make any criticisms he saw fit, broad or detailed, just as he can now, recommending rejection or contributing to improvement of the paper in any manner he deemed appropriate. However, if he so chose, refereeing could be confined to broad judgment of the worthiness of the work reported, with the referee not having to feel as materially responsible for a paper as he does, or should, nowadays. His main responsibilities would be to satisfy himself that the work was reasonable and complete, that the abstract correctly represented the full-length paper and that the details of that paper were well laid out and bore up under reasonable surveillance. Following publication, it would rest upon the specialists, those who will read the paper in its full length, to assess its merits so far as technical content and detail is concerned. After all, they and the author are the people to whom the value of the detail is of prime concern. And they are also the only people for whom judgment of the quality of the paper is of material importance.

Certain consequences of this kind of refereeing are easily forecast. First, it could (and should) be achieved more rapidly than is most present-day refereeing. This would carry the advantage that referees would not have to spend so much time on the job, which is nowadays usually a thankless chore occupying many hours. Also, authors would not have to suffer lengthy delay in getting results published, and the general dissemination of knowledge would be hastened. Second, it might be easier to get an extended abstract published than it is now to get a paper published. This, of course, might be a nightmare to the high standards of publication that most journals now aspire to. However, the published form of each paper would be of limited length and the burden of judgment of the merit of a paper would rest more properly where it should - not upon editorial staffs, but on the specialists in the topic concerned. And these would be the people getting the full-length paper and making good use of it. Others, with no initial interest in details, would benefit from clearly distilled abstracts that would provide better information than do to-day's summaries, and these same people would not be burdened with the high costs of publishing lengthy detail as is done to-day. It is likely that libraries would want to have the full-length version of all papers, for which microfilm would probably be the appropriate mode of storage. This might also be true for each journal's repository of full-length articles.

Even if publication of an extended abstract became easier to achieve than is the publication of a paper to-day (as some will surely suggest, and perhaps with just cause) it is this very fact that might alleviate some of the more severe cases of the publish or perish syndrome. Nevertheless, there is a possibility that if refereeing became less detailed than it now is, unwarranted claims to new results might get published in extended abstracts more readily than they do in to-day's regular papers. However, a safeguard against this could be provided for by having a policy of publishing evidence that results are in error.[1/]

[1/] I am grateful to H. O. Hartley for this suggestion.

Publication of research results in the form of extended abstracts only should also reduce the present-day atomizing of published results. Journals would have no need for encouraging short papers and referees could ensure that atomizing got discouraged. However, embarking on a policy of publishing just extended abstracts would be fateful if only one journal tried it, because most of that journal's potential authors would probably direct their work elsewhere. At the very least, some group of journals within any discipline would have to co-operatively undertake such a policy together. If this suggested mode of publishing research results, or any variant of it, bears merit, at least it might in the first place be worth an experimental try-out for a limited number of years.

The second aspect of the suggested amendment to present publication practices would be to elicit and publish more, full-length, review-type, expository and/or survey papers. They are desperately needed. But they do not seem to be forthcoming. One reason for this may be that refereeing policies for these papers (call them review papers) may currently be no different from those for research papers. If so, potential authors of review papers may be reluctant to undertake the solid work involved, only to be questioned by referees on matters of detail in the same way as are authors of research papers. Perhaps a clearly stated and different policy for refereeing review papers could promote their preparation. It would amount to inviting well qualified people to author such papers, and having them refereed in terms of their general content and overall treatment of their topics, leaving responsibility for all detail entirely on the authors' shoulders. After all, presumably an invited author would be invited only if it was felt that he would shoulder this responsibility reliably.

Nowadays, when a research paper is published an editor, through his associate editors and referees, is in some sense saying "This is a good paper, we think it should be read". But when publishing a review paper by an invited author a more reasonable attitude might be "This is a good man and this is what he says". Based on this attitude the contents of a review paper would be much more the responsibility of the author than is the case to-day for authors of research papers. Concern may be voiced that such an attitude might lead

to biased review papers. However, refereeing would surely ensure that the bias was never excessive. Furthermore, it must be admitted that by definition a review paper almost has to be biased to some extent, for it is only one person's opinion of the present status of his chosen topic and, with scientific literature continuing to expand, it is almost impossible for one person to categorically know everything about a topic. But if the choice of author is judicious the bias should be minimal. Rather than worrying about such biases we should encourage the preparation of more review papers that we may benefit from the wisdom and perspicacity of the people well versed in their fields who would be invited to write such papers. This would be beneficial to the extent of having 2, 3, or maybe more, review papers in every issue of a journal, even with the same topic sometimes being reviewed, by different authors, within perhaps 4 or 5 years of one another. Reviews of topics on which numerous research papers had appeared in any quinquennium would provide a welcome up-dating of the topic both for the people actively engaged in research and for those teaching it. Indeed, for many people this may well be the only hope they have of keeping up with the advances being made in their fields of interest. And it should satisfactorily augment, for the non-specialist, the publication of extended abstracts.

This suggestion for radically changing current refereeing and publishing procedures is probably as full of weaknesses as is the present system. But since some of the present weaknesses are quite serious, it is surely beholden upon scientists, by their interests in experimentation, to at least <u>try</u> something else. Maybe a discipline having only a few major journals could undertake such an experiment, remembering that the problems that it faces regarding the information explosion are miniscule compared to fields like physics, chemistry or nutrition, for example. There, the numbers of people and publications may exceed those in a smaller discipline by at least one order of magnitude. And who knows, if experimentation with a new form of publishing were successful in a small discipline, it could become a leader in helping solve the same problems in larger disciplines.

Acknowledgment

Discussion with numerous colleagues has prompted preparation of this paper, but for the expression of the ideas therein I am solely responsible. Particular thanks go to D. L. Solomon for the idea of publishing extended abstracts, about which we have had lengthy discourse.